EFFECTIVENESS OF CHIROPRACTIC AND PHYSIOTHERAPY IN THE TREATMENT OF LOW BACK PAIN: A CRITICAL DISCUSSION OF THE BRITISH CLINICAL TRIAL

To the Editor:

Assendelft et al. (1991) are to be complimented on their critique of the Meade Study (1). They have indeed shed light on areas of the Meade Study which add a new dimension in interpreting and evaluating its meaning. However, there are several areas of the Meade Study which Dr. Assendelft did not address that I feel compelled to bring to your reader's attention.

First, as pointed out in my letter in The Lancet (2), the Meade Study was not a comparison of conventional hospital outpatient management and chiropractic in the treatment of low back pain. I was disappointed that Dr. Assendelft, throughout his article, perpetuated that false premise. As a physiatrist I can tell you spinal manipulation is hardly ever included in the outpatient hospital treatment of low back pain or any other condition, at least in our country. The Meade Study was, in fact, a randomized clinical trial comparing different manipulative techniques performed by manipulators with very different orientation, training, educational back-
ground, and manipulative skill. The patients in the trial were conveniently labeled sufferers of "mechanical low back pain," which essentially means low back pain of unknown etiology. The chiropractors manipulated 99% of their patients, while physiotherapists manipulated 84% of theirs. We do not know what, if any, diagnostic spinal examination was carried out to determine if the patient was a suitable candidate for manipulation, nor do we know the criteria employed for the selection of a specific technique for that particular patient.

A possible explanation of why the chiropractors obtained better results than the physiotherapists may reside in the principal findings of the New Zealand Report of 1979 (3). The New Zealand Report is probably the most exhaustive study of chiropractic ever carried out by a government commission. One of the principal findings of the commission was that "no other health professional is as well qualified by his general training to carry out a diagnosis for spinal mechanical dysfunction or to perform spinal manual therapy as a chiropractor." The commission also concluded, and this is relative to the Meade Study, "Although the precise nature of the biomechanical dysfunction which chiropractors claim to treat has not yet been demonstrated scientifically, and although the precise reasons why spinal manual therapy provides relief has not yet been scientifically explained, chiropractors have reasonable grounds based on clinical evidence for their belief that symptoms of the kind described above can respond beneficially to spinal manual therapy" (3).

In 1956, long before the New Zealand Report and the Meade Study, Dr. Boje, Senior Physician of the Rigshospital in Copenhagen, Denmark, wrote in The Journal of the Danish Medical Association pleading for cooperation between qualified chiropractors and physicians (4). Dr. Boje stated, "If the physiotherapists want to use manipulative treatment, they ought to get an education just as thorough as that of the chiropractors."

Interestingly, in an Australian study (5) which compared cervical manipulation performed by a chiropractor, a medical practitioner and a physiotherapist for the treatment of migraine, the patients that were manipulated by a chiropractor reported a greater reduction in pain associated with their attacks.

Manipulation for low back pain has been used empirically since ancient times. In spite of the explosion of medical knowledge and technology (e.g., heart transplant, the performance of invasive diagnostic studies and surgical repairs on a fetus in utero, etc.), manipulative treatment of low back pain remains empirical. In fact, at the present time we are unable to identify which specific pathologies or spinal biomechanical derangements would possibly respond to manipulation. Manipulation for low back pain may have to join the long list of therapeutic interventions which are not susceptible to scientific proof at the present time.

Unfortunately, the present state of the art causes us to remain in a state of ignorance regarding which musculoskeletal derangements would best respond to spinal manipulation. In the Meade Study, chiropractors manipulated 99% of their patients who suffered symptomatic low back pain of unknown etiology. It is, of course, highly improbable that spinal manipulation was the treatment of choice for all 99% in the trial who received it. Once those specific biomechanical abnormalities which are responsive to manipulation could be identified, we would expect the positive results observed in the Meade Study to be greatly improved.

The low back pain that the patients in the Meade Study were complaining of may very well have been secondary to spondylolysis, disk space narrowing, Schmorl's nodes, lumbarization or sacralization and spinal osteophytes. The above are radiographic findings, and to complicate matters more, do not have to necessarily be causally related to low back pain, as these X-ray findings are nonspecific and are observed equally often in patients with or without low back symptoms (6).

Incidentally, computed tomography (CT) and studies (7) also demonstrate disc bulge or herniation in an unexpected high percentage of asymptomatic patients, which may cause confusion in determining causality to low back pain.

Further interpretative difficulty of the Meade Study is introduced by Dixon (8), who claims anyone caring for patients with low back pain will help 70% of patients. Headache and backache are common psychosomatic complaints. The placebo affect in these patients is enormous. Unfortunately, the Meade Study did not have a control group receiving placebo treatment to compare with the other two treated groups.

Manipulation is not alone seeking a scientific explanation of why, when, where and how it works. Medicine is also seeking scientific evidence to support many empirical treatments. Katz et al. (9), treating low back pain by continuous inclined pelvic traction, claimed to have obtained good results, but when asked why it was effective (10), were only able to speculate that this treatment "might have some effect on reduction of the intra-articular pressure between the facets, and may be even responsible for the reduction of intradiscal pressure." He further commented, "We do not have laboratory evidence or CT scan data to support these latter hypotheses. The only evidence is the improvement in the patients' clinical signs." Certainly, chiropractors could understand and identify with Dr. Katz's explanation.

The Meade Study has demonstrated that spinal manipulation performed by a chiropractor is more effective for low back pain of unknown etiology than when the patient receives manipulation by a physiotherapist. Hopefully, further research will help us understand why, where, when and how spinal manipulation works.

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REFERENCES


In Reply:

We thank Dr. Brien for his comments and additional remarks to our review article (1,2). The Dr. Brien’s letter clearly demonstrates the problems in properly interpreting the results of the British Medical Research Council (MRC) study (3). Dr. Brien was disappointed that throughout the article we used the terms “chiropractic” and “hospital outpatient treatment” as labels for the contrast evaluated in the study (3). Dr. Brien was disappointed that throughout the article (1), we explained that in our opinion it is impossible to identify the responsible component for the difference in outcome found between the two treatment groups. The type of manipulation was only one of the five candidate causes. The point is that the design of the MRC study (3) permitted multiple interpretations afterwards. For instance, somebody interested in health care research could conclude from this study that (regardless of the type of treatment given) private practice is superior to nationalized hospital outpatient treatment.

The fact that 84% of the physiotherapy patients were treated with manipulation was a surprise to us as well. If this percentage substantially deviates from the British routine (which we actually don’t know), one might fear that the manipulations have been performed by physiotherapists relatively inexperienced in this kind of treatment. In that case, the only conclusion could be that under these trial conditions the wrong physiotherapists performed the manipulations, and certainly not that physiotherapeutic manipulations are less effective than chiropractic manipulations.

We share Dr. Brien’s concern that one is not able to judge the diagnostic approach of the patients and the criteria used by the individual therapists to select the type of treatment. Dr. Brien doubts the significance of particular findings on plain X-rays. Plain X-ray taking still seems to be one of the cornerstones of chiropractic patient management (4). Recently, Phillips critically assessed the motives for X-ray taking by chiropractors (5). He concluded that the importance of X-rays for biomechanical evaluation, both as a diagnostic as well as a monitor for therapeutic progress, has not been fully clarified yet.

We realize the important role the New Zealand Report (6) has played in the acceptance of chiropractic. The scientific support for the efficacy of chiropractic in the report consisted of anecdotal patient histories, some (uncontrolled) case series and five randomized clinical trials (RCTs) (6,7). Because uncontrolled studies have major methodological flaws, the efficacy of chiropractic should be determined from RCTs (7,8). Only one out of the five RCTs discussed in the New Zealand Report involved chiropractors (namely, the migraine trial of Parker et al. (9). This means that the four nonchiropractic RCTs in the report cannot be regarded as evidence for Dr. Brien’s opinion that chiropractic manipulation is superior to other types of manipulation (on the contrary, it rather seems to illustrate the opposite). In addition, the commission interpreted the results of the only chiropractic RCT in such a manner that the investigators (Parker et al.) later dissociated themselves from the commission’s conclusions about this trial (10).

Dr. Brien discusses the interpretation problem introduced by the lack of a placebo control group in the MRC trial (3). It is almost impossible to establish placebo manipulation in a “pragmatic” trial such as the MRC (3). A so-called waiting list control group seems to be the second best, but most feasible choice to monitor natural history of complaints and participation bias (bias due to the extra attention patients get when they participate in a study) in a pragmatic trial. Recently, we completed a proposal for an RCT comparing chiropractic and physiotherapy for tension headache, which includes such a waiting list control group (11). The inclusion of this latter group will enable us to estimate the magnitude of the contribution of natural history and participation bias to the treatment results of the chiropractors and physiotherapists.

The conclusions that can be drawn on basis of an RCT like the MRC study (3) are heavily dependent on the methodological quality of the RCT. We assessed this methodological quality of available chiropractic RCTs on low back pain (n = 5) recently. The results of this assessment will be published in this journal soon (8). We think more chiropractic RCTs with a better research methodology are clearly needed. Fortunately, at present there are a number of chiropractic RCTs in a preparation, execution or reporting phase (7). We sincerely